Variable Stars.

Star.	R.A. h. m.	Decl. ° ′			h. m.	
Algol	3 1·0	40 32 N.	Sept.	14,	2 3	*m*
W Tauri	4 21·7	15 51 N.	,,	11,		*m*
X Boötis	14 18·9	16 49 N.	,,	10,		*m*
U Coronæ	15 13·7	32 3 N.	,,	9,	23 43	*m*
U Ophiuchi	17 10·9	1 20 N.	,,	8,	1 33	*m*
		and at intervals of			20 8	
X Sagittarii	17 40·6	27 47 S.	Sept.	9,	1 0	*M*
W Sagittarii	17 57·9	29 35 S.	,,	14,	23 0	*m*
U Aquilæ	19 23·4	7 16 S.	,,	12,	3 0	*m*
η Aquilæ	19 46·8	0 43 N.	,,	8,	1 0	*M*
X Cygni	20 39·1	35 11 N.	,,	8,	21 0	*M*
T Vulpeculæ	20 46·8	27 50 N.	,,	9,	21 0	*M*
R Vulpeculæ	20 59·5	23 23 N.	,,	11,		*M*
δ Cephei	22 25·1	57 51 N.	,,	10,	22 0	*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

FROM advices just received from Queensland the *Colonies and India* understands that Sir William MacGregor left Port Moresby, for the ascent of Mount Owen Stanley, on April 19, in an open boat, with a party of fourteen for Vanapa River, 30 miles westward. He arrived safely, and pushed the boat up river for eight days, during which period he encountered many difficulties in crossing rapids and dragging the boat over rocks. When he could get no further he camped on the left bank of the river and sent Mr. Cameron (his secretary) back to Port Moresby for supplies, with native carriers to cross the mountain. Mr. Cameron returned with two boats loaded with provisions, thirty natives, and six Polynesians. All being ready, on May 17 the party, comprising forty-two men, left the camp, all packing, and the Governor taking the heaviest load. There were only four whites in the party. They crossed Mount Gleason at Eyton Junction, and then shaped a course north-east by east. At a height of 175 feet they crossed Mount Gunbar, next Mount Kulwald. Mount Belford was crossed at the Joseph River, after which they descended to the Goodwin Spur, and saw the first native house at Goodwin's village, Mount Musgrave, where they camped, the Governor going ahead with four Polynesians and seven natives; then ascended Mount Musgrave for over 7000 feet to Vanapa River and Mount Knutsford Range, over rough country; they followed a spur leading west. After three days' march they descended the spur, and began the ascent of Mount Owen Stanley on June 9, reaching the top on June 11 and 12, returning to Mount Musgrave on June 16. All hands started homewards and arrived at River Camp on June 22. They left on June 23, visited a mountain village on June 24, and met the steam launch from the *Merrie England*, which was searching for the Governor's party, and took them in tow. They arrived at Mana Mana on the 25th, and were taken on to Port Moresby, where the party landed after two months, all well. The only death that occurred was that of a native. The country traversed was very mountainous, and no table-land was discovered. Of the geological formation the country is mainly decomposed slate, granite, and quartz, with no sign of gold. Specimens of rock were collected by the Governor. The climate to 8000 feet is moist, above that dry and bracing. Natives were met on only two occasions, and were extremely friendly. They were stout well-built men, but no women were ever seen. Cultivation paddocks were fenced in. Potatoes, yams, and sugar-cane were plentiful, as also was tobacco. Natives, who were devoid of warlike implements, paid particular attention to head-dresses made of shells procured from the natives on the eastern coast of German New Guinea, who were showing friendly communication. Across the Owen Stanley Range the Governor collected many specimens of new plants, among others being some beautiful yellow rhododendrons, which he has since sent to Melbourne to Baron Von Mueller for report. A great number of new grasses in large patches were discovered. At Mount Victoria (Goodwin) he secured several new birds and one animal, which was something like a native bear, but had a long tail and dusty-brown collar and black extremities. The extreme length was 3 feet 6 inches, of which the tail was 1 foot 6 inches. There were five claws on all the feet, the tail was bushy, and it was estimated that the weight of the animal was 40 pounds. The birds of the lower altitudes were the same as those before seen, except as to a new paradise bird similar to the Great Epimachus. Sir William procured a female *Astrachia stephania*, the only male bird of that species being in the Museum, Berlin. The Governor procured several new small birds at Mount Victoria, including the identical English lark. Unfortunately, they were eaten by the Polynesians. Entomological specimens were obtained, including a milk butterfly. Only a few were captured.

ACCORDING to a *Times* telegram from Zanzibar, the Sultan has signed a concession to the British East Africa Company, of Lamu and the Ben-Adir coast, embracing all his territory from Kipini to Mruti. The concession embraces the administration and government of the island and port of Lamu, and of the northern mainland ports of Kismayu, Brava, Magadisho, and Warsheikh. The company's jurisdiction is thereby extended from the River Umba, in the south, to the port of Warsheikh in the north, an extent of about 700 miles of coast-line. Lamu is, next to Zanzibar and Mombasa, the most important port on the East African coast, and commands the trade of the mainland south of Kismayu, and that of the fine waterway of the Tana. It has been for years past the seat of a flourishing commerce, which is mostly in the hands of British Indian subjects, and it is a port of call for the British India Company's steamers.

OUR SENSATIONS OF MOTION.[1]

WE may distinguish two quite different kinds of sensation of motion, active and passive. When we walk or run or row, we use our muscles, and this use of our muscles is the cause of our motion, and also the cause of special sensations which may in a sense be called active sensations of motion. But we have other sensations than these connected with motion. For, if we are carried, or rocked in a boat, or dropped from a height, we are not only moved, but we are conscious of a very well marked sensation which we may call a passive sensation of motion. When we move ourselves we feel both kinds, and it is difficult for us to analyze what we feel and distinguish between our sensations as movers and our sensations as moved. It is to our passive sensations of motion that I wish to direct your attention to-night, and as these can best be examined in cases where they are not complicated with the other kind, we shall confine our attention almost exclusively to passive motion—that is, to cases where we are moved without any exertion of our own muscles. Now the first thing I have to say is in at all events apparent contradiction to the title of this lecture: it is that we have no direct sensation of motion as such. That this is so will be at once obvious if we consider the fact known to all, that we are at this moment being moved with very great velocity through space. We know that this is so, astronomers can prove it, but we are so perfectly unconscious of it that I dare say most of us here could not point the direction in which we are moving; in fact, as we are ignorant of the direction and rate of motion of the great system of which our solar system is a part, no one can say how fast and towards what point in space we are travelling. What we are conscious of is change of motion. It is because the motion of the earth is so steady, because, although very rapid, its changes are very slow, that we do not feel it.

There are two altogether different ways in which a body can be moved. These have been called respectively translatory and rotational. In translatory motion the body is always similarly oriented. Thus, if we consider motion within so small a part of the earth's surface that we may neglect the earth's curvature, such an object as this desk is subjected to purely translatory motion if we move it thus, so that the same side always looks up, and the same side always looks east. Rotational motion involves a change of orientation, and is rotation about an axis. This axis may always be the same, or it may change, and the change of axis may be abrupt or may be continuous. Most of the motions which we observe are made up of both kinds. When we travel by rail—always supposing that we sit still in the carriage—we are subjected to a purely translatory motion only when the train is running along a perfectly straight piece of the line. When it goes round a curve, we are—always supposing we sit still—subjected to rotation as well as to translation; because our face no longer continues to look in the same direction, but, as long as the train is running on the curve, continuously changes the direction in which it looks.

Let us examine what we feel when we are passively subjected to purely translatory motion. As long as the motion is steady, unchanged in speed or in direction, we feel nothing, as has been pointed out already. Let us look at the case when the speed changes, the direction remaining the same. We have to consider separately three different directions: (1) horizontal, (2) up, and (3) down, because we shall see that our sensations are different in these three cases. With change of speed in horizontal motion we are all familiar. The starting and the stopping of a train or of a steamer give us ample means of studying it. We all know the jolt of a badly started train. What we feel in such a case is, mainly at all events, due to our body being jerked forward or backward, according as we are sitting with our back or our face to the engine. But if the train is carefully started we find that our rate of motion may in a very short time be changed from nothing (relatively to the earth) to, say, thirty or forty miles an hour, without our feeling anything but the up and down rattle due to the slight unevenness of the rails. And the same was the case till comparatively lately with stopping. Now, however, since the introduction of the continuous brake, a train can be so rapidly stopped—its rate of motion, that is our rate of motion when we are in it, can be so very quickly changed from, say, sixty miles an hour, to nothing—that we do feel a strange, not altogether pleasant sensation. Experience has taught us what that sensation means, but at first it was so novel that experience was necessary to interpret it. It is not a sensation of jolt, the change though rapid is not abrupt. What we really feel, although it takes some amount of careful observation and some thought to see this clearly, is that the direction of the vertical, the direction in which a body falls, the direction in which our body presses has been changed. We feel this most distinctly if we are standing when the brake is applied; we feel that if we do not take means to prevent it, we shall fall over, and we prevent this by bringing our body into the line of the new vertical. Our feeling of unsteadiness depends on our uncertainty how long the new state of matters is to last. It lasts as long as the speed is being changed at the same rate, and the deviation of the new from the real vertical depends on the rate at which the speed is being changed. Our perception of deviation from the vertical is pretty acute. Most of us can tell a line to be off the vertical when it is inclined only a few degrees to it. In ordinary cases we have extraneous help in judging. We have walls or chimneys, known to be vertical, or surfaces known to be level, with which to compare, but even when we have no assistance of this kind we are not often far wrong. It might be supposed that it is the pressure of our body on the floor or ground that gives us the idea of the vertical, but that idea still exists in cases where we can feel no such pressure. If our body is supported in water, or entirely submerged, as in diving, we still have a very distinct, and fairly accurate notion of up and down, although in such cases, as our body is very nearly of the same density as water, the resultant pressure on it is small. We shall see in a little a possible explanation of our sense of the vertical. When the train in which we are travelling runs quickly round a sharp curve, we feel something very like the sensation just described. And indeed it is due to a perfectly similar cause; the apparent vertical is the direction of the resultant of the force of gravity and the centrifugal force, and is, as every engineer knows, more inclined to the real vertical as the curve is sharper and the speed of the train greater. In this case the sensation is complicated, because the motion is not one purely of translation, but, as already pointed out, is compounded of translatory and rotational motion.

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[1] Armitstead Lecture delivered in Dundee, by Prof. A. Crum Brown.

Let us now look at cases of up and down motion. As we can study horizontal motion in the railway train, so we can up and down motion in a lift. Here also we see that it is change of motion which we really perceive. For, once the lift is started, and is moving smoothly and uniformly, either up or down, we are quite unconscious of the motion. It is the start and the stop, or the quickening or slowing of the motion only that we feel. And the stopping of the upward motion produces exactly the same feeling as the starting of the downward motion, provided they are equally smooth and free from jerk. It is easy to see what are the physical conditions here. Just as the acceleration in a horizontal direction inclines the apparent direction of gravity, so acceleration up increases, and acceleration downwards diminishes, the apparent intensity of gravity. If the lift *fell* down, unrestrained, its inmates, during the short time the experiment would last, would have no sense of the force of gravity at all. An object dropped from the hand would not fall down to the floor, because the floor itself would be falling at the very same rate as the object. And what is true in this extreme case is true also in a measure in all cases of downward acceleration. But only in cases of *acceleration*, for, however fast the lift goes down, if it moves uniformly, without change of speed, the bodies of those in it press on its floor exactly as if it were at rest. Similarly, upward acceleration increases the apparent force of gravity. The physical conditions, then, of our perception of acceleration of translatory motion in any direction are change in the apparent direction, intensity, or both, of the force of gravity. It is a strange and interesting fact that our perception of downward acceleration—that is, of diminished force of gravity—is more acute than that of acceleration upward or in a horizontal direction. We feel the starting of the lift as it goes down, and its stoppage when it has come up, much more distinctly than the start on the way up or the stop at the end of the journey down. And when we are rocked in a rolling steamer, it is the beginning of the downward move that is most perceived.

Having now discussed the phenomena of our sensations connected with translatory motion, let us examine what our experience is when we are turned round, or subjected to rotational motion. We execute such movements every minute of our waking life. But as with translational motion, so, and even more, with rotational motion, it is impossible to analyze our sensations when they are complicated with what we feel we do. And in the case of rotation a very serious complication is introduced by our seeing how we are being moved. So that, to make a strict examination of our sensations in this matter, the observer must place himself blind-folded on the rotating apparatus, and be passively turned round. Or, as in Prof. Mach's very ingenious experiments, a small hut with translucent paper windows may be placed on the turn-table for the reception of the observer. Just as we can move, or be moved, right or left, backwards or forwards, up or down, so we can be spun round about a fore-and-aft, a right-and-left, or an up-and-down axis, and about each of these axes either the one or the other way round. It is plain, however, that we can get simple results only in the case of rotation about a vertical axis, because otherwise a great complication would be introduced by the varying position of our body relatively to the direction of gravity. We shall see that we can get everything we require with rotation about a vertical axis. Here we find, as in the former case, that it is only change of motion that is perceived. The observer sits on a chair on the turn-table, his eyes are bandaged, and an assistant gives the table a steady, uniform rotation. At first the observer feels the turning quite distinctly, but after less than a whole revolution the sensation becomes very indistinct, and, while the turning still continues at the same rate, soon disappears altogether. If the rate of turning is now increased, he feels it begin again, all that he perceives being the increase; his perception of that also soon dies away, so that in a short time he may be spinning rapidly round, while he feels completely at rest, and is only aware that he has been gently turned round a little, two or three times. But if you now stop him he feels a turning round in the opposite way to that in which he really was turning, the fancied rate of turning being, at the moment of stopping, that of the real turning which has just been stopped. This imaginary rotation dies away, exactly as the sensation of the real rotation did. Now, *very nearly* the same thing takes place, whatever is the position in which the head is placed during the experiment, *if the head is kept rigidly in the same position during the whole of the experiment.* Let us look at a case in which the position of the head is not kept the same during the experiment. The observer sits on the table, with his head inclined to one side, so that the line from ear to ear is vertical. He is now turned uniformly round; as before, he feels the turning at first, but as the uniform turning goes on the sensation dies away. When he feels perfectly at rest, let him give the word to stop, and at the same instant raise his head into the ordinary position. He will now feel as if he were being turned about the line from ear to ear—that is, now, about a horizontal axis. If his right ear was down when he was actually being rotated, and if the turning was with the hands of a watch lying with its face up, then the imaginary rotation will be the opposite way round—he will feel as if his head were going forward and his feet back. This sensation will last only a short time, but there is a risk in trying the experiment, that the observer may try to correct this alarming overturn by throwing himself backwards; if he is nervous it may therefore be as well to have him strapped to the chair. Whatever line in the head we make vertical while the real rotation is going on—that is, whatever line in the head we make the axis of the real rotation—that line is the axis of the apparent rotation which we feel when the real rotation stops, however we may move our head at the time of the stoppage. There is a practical joke depending on this principle, which I have seen played. The subject of the joke, who ought, of course, to be a person not conversant with the laws of the sensation of motion, is asked to hold a poker upright on the floor, and, placing his forehead on the top of it, to walk three times round it, rise up, and walk across the room. His march round the poker is a rotation about the fore-and-aft axis of his head; when he rises up he feels the contrary imaginary rotation, about the same axis, now, of course, horizontal, so that, if he went round with the sun, he falls to the right, and to the left if he went round the other way. In a very interesting experiment with the turn-table we have a combination of real and apparent rotation. Lie down on the table, say on the left side, so that the left ear is vertically under the right ear, so making the right-and-left axis of the head the axis of rotation; let the table be turned round at a uniform rate, wait until all sensation of rotation has ceased, and then, while the uniform turning is still going on, roll yourself over on your back. You will then experience a very startling sensation. The new axis of rotation of the head is the fore-and-aft one; there was no rotation about it before, therefore you feel that real rotation, the real rotation about the old axis—right-and-left—has just ceased, therefore you feel the imaginary opposite rotation. The sudden occurrence of the combination of these motions, felt as real, resulting from a cause so seemingly inadequate, rolling from your side to your back, gives rise to an almost dreadful sense of insecurity.

Thus then in rotational as in translational motion it is change of motion, what is technically called acceleration, which we perceive. There are two questions which naturally arise in this connection: (1) What is the use of this sense? and (2) What is the organ of this sense, and how does it work? What is the use of it? Everyone will, I am sure, admit that it must be of great use to us to have a constant knowledge of the direction of the vertical; to have, as it were, a private level of our own, which we cannot mislay. As to our sense of rotation, it is to it chiefly that we owe what we call our knowledge of the airts; it enables us, as we walk about on winding roads, or through narrow crooked streets, to retain some idea of the real directions. But the chief use, no doubt, of the sense of rotation is to enable us to control and regulate the rotatory movements of our head—movements we are constantly making as we look about us. It may be asked, in these short quick movements of the head, where is the secondary sensation of turning the other way which I described? We never experience it at all. Mach has very clearly, and with great penetration, explained how this comes about. These quick movements, our habitual movements of rotation, are so short in their duration, that during them we do not come to feel that our head is at rest. The sense of the original real rotation is still vivid when the rotation is stopped, so that the secondary sensation of an imaginary motion the other way round merely annuls the primary sensation, puts an end to it when the real rotation stops. Without such a stopper of sensation we should go on feeling the rotation for a short time after it was done.

What is the organ of this sense? There is in our head a very remarkable organ which has been for long a puzzle to physiologists, an organ which is found not only in our heads, but also in the heads of all mammals, of all birds, and of all but the very lowest fishes (and even in the very lowest fishes it occurs in a less developed form). This organ is so closely related in position to the organ of hearing, that it was long supposed to be a part of it, and we shall see what attempts were made to explain it as an organ of hearing. I shall give as short a description of it as is compatible with making it plain how it can act as the organ of the sense we have been considering. I must at the same time confess that in some points our knowledge of the matter is still imperfect, and that much has still to be done before we can explain its action as fully as we can that of the eye, for instance.

The organ in question is lodged in a bony cavity continuous with that which contains the organ of hearing, and for this reason was long, and perhaps by some is still, regarded as itself having something to do with the perception of sound. This cavity in the hardest bone in our head consists of four parts—the vestibule, and the three semicircular canals. The vestibule is an irregular chamber, in man about $\frac{1}{4}$ of an inch long and $\frac{1}{7}$ of an inch broad and deep. In its walls are five openings leading to the semicircular canals. These are tunnels in the bone having an elliptical or circular section, and opening at each end into the vestibule. The central line of each canal lies nearly in one plane, which we may call the plane of the canal. At one end of each canal there is an enlargement called the ampulla. The planes of the three canals are approximately at right angles to one another. The canals are named from their position—the horizontal, the superior, and the posterior; the two latter unite at their non-ampullary ends before joining the vestibule, so that there are five and not six openings into the vestibule from the canals—three ampullary, one for each canal, and two non-ampullary, one for the horizontal, and one common to the superior and the posterior canals. The plane of the horizontal canal is nearly horizontal in the ordinary position of the head in all animals, and is therefore at right angles to the mesial plane: the planes of the two other canals make nearly equal angles with the mesial plane.

In the bony labyrinth just described there is inclosed a membranous labyrinth of a generally similar form. It consists of the utricle, lodged in the vestibule, and of three membranous canals, each furnished with a membranous ampulla. The membranous labyrinth does not fit tight into its bony case. The utricle is much smaller than the vestibule, which contains, besides, the saccule, an organ connected with the cochlea; and the diameter of the membranous canals, except at the ampullæ, is much less than that of the bony canals. The membranous ampullæ, on the other hand, nearly fill the bony ampullæ. The entire cavity is thus divided into two spaces, one within, the other around, the membranous labyrinth; each is filled with a liquid, the endolymph and the perilymph. The nerves are distributed to one spot in the utricle, and to a crescent-shaped ridge near the middle of each ampulla. The nerves end in hair-cells, the hairs of which project into the endolymph. The *macula acustica*, the spot in the utricle to which nerves are distributed, is covered with a gelatinous layer in which are embedded small crystals of carbonate of lime.

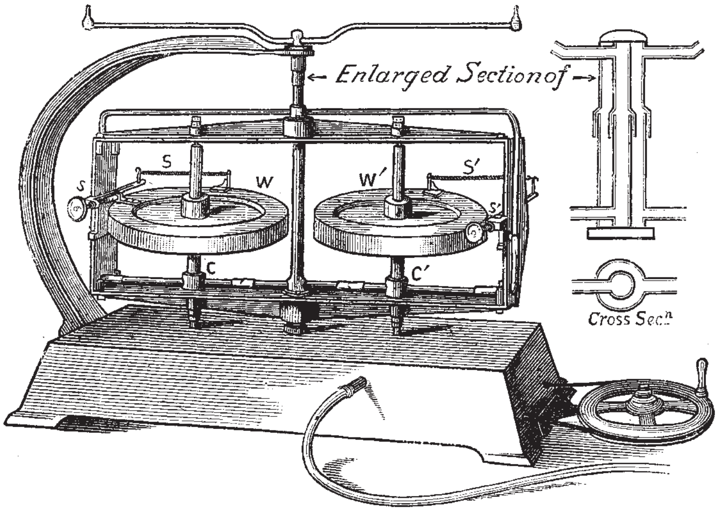
Everyone must see that an apparatus so purpose-like in its arrangement must have a use, and this use must be one applicable to all the higher animals.

It was long supposed that it had to do with our perception of the direction from which sounds come to us. The idea is not unnatural, and is obviously derived from the nearness of the apparatus to the organ of hearing, and from the relation of its form to the three dimensions of space. No explanation has ever been given how it could serve this purpose; and we can easily show that it does not do so by experimentally showing that we have no means of ascertaining the direction from which a sound comes except by two or more simultaneous or successive observations. If a sound is heard louder in the right ear than in the left we conclude that it comes from the right, and by turning round the head we soon get a sufficient number of observations to enable us to judge of the exact direction. If a short sharp noise is made at a point equidistant from the two ears, we do *not* know the direction from which it comes unless we see what causes it.

But the apparatus is admirably fitted to act as the organ of the sense of rotation, or rather of the sense of acceleration of rotatory motion. Let us first consider the action of one canal. If the head is rotated about a line at right angles to the plane of the canal, with the ampulla leading, you will see from looking at the diagram that there will be a tendency of the endolymph to go from the utricle into the ampulla, and of perilymph to go from the space between the bony and the membranous canals into the utricle. These will conspire to stretch the membranous ampulla where the nerve-endings are, and we can easily see how this will stimulate the nerves, and send a message to the brain. But this stretching will not take place if the head be rotated the other way about. In that case the tendency of the fluids will be in the opposite directions, and will rather make the ampulla less tightly stretched, and we can readily suppose that this may not stimulate the nerves, and no message will be sent to the brain. One canal will thus be able to give indications of rotation about one axis, in one of the two ways round. But for each axis we have two canals, one turned the one way and the other the other. And as by means of three rectangular axes we can represent any rotation, so any rotation will be perfectly recognized by means of the six canals. That this is actually the function of this organ is further proved by the effects of injury or disease. One ear is sometimes attacked by inflammation while the other ear is unaffected. In such cases the patient suffers from persistent vertigo—that is, sense of rotation where no real rotation occurs. This is, at least, one form of what is known as Ménière's disease, so-called from the name of the physician who first drew the attention of medical men to it. Now it is obvious that, if the six canals act in the way I have indicated, the pathological irritation of the three canals of one ear will produce a sensation of rotation about an axis the position of which we can foretell from the relative positions of the planes of these three canals. And clinical observations on persons suffering from this disease show that the vertigo actually is about this axis. Deaf-mutes are persons who from a very early age have had no sense of hearing at all. This condition may arise from the imperfect development of the organ of hearing, or from its early destruction by disease. In either case it often happens that the organs we have been discussing, from their nearness to the organ of hearing, are involved in the mischief, and are also imperfectly developed, or destroyed by disease. Deaf-mutes have therefore not unfrequently the semicircular canals in a state unfit for use. Experiments have been made on deaf-mutes with the object of testing the accuracy of their sense of rotation. Those who have made these experiments report that many deaf-mutes are insensitive to rotation. If these observations are confirmed, the theory I have just been explaining will receive a very great support.

In order to illustrate this theory, and to show that the principle on which it is based is a sound one, I have devised a sort of working model which I shall now show you. I may say that when I accepted your invitation to lecture here, and had selected the subject of our sensations of motion, the idea occurred to me of making a sort of working model of the semicircular canals. The difficulty was to find an instrument-maker who could help me over the obstacles which always lie in the way of a designer who is not himself an engineer. I take this opportunity of thanking Mr. Alex. Frazer for his help in this matter. He at once understood what I wanted, and so gave me the use of his skill and experience that the instrument here is exactly what I intended it to be, and a great deal better than my most sanguine hopes.

The model, as perhaps you will better see from this somewhat diagrammatic drawing than from the machine itself, consists essentially of two heavy wheels, placed side by side, with their axes parallel, in a frame which itself can be turned round about an axis parallel to that of the wheels. These heavy wheels correspond to two parallel canals, say the two horizontal canals. As it is the inertia of the fluid in the canals which enables them to work, so here it is the inertia of the heavy wheels. Each wheel has a stop, which altogether prevents its turning, in one way round, beyond a certain point. The one wheel is thus checked in turning the one way, the other in turning the other way. Each wheel is just held against its stop by a spring which is stretched when the wheel turns away from the stop. Each wheel with its stop and spring is as nearly the mirror image of the other as it could be made. When I turn round the frame, both wheels tend to lag behind the rotation of the frame, on account of their inertia. One of them cannot lag behind at all because of its stop, and the other cannot lag much behind because of its spring; the stronger we make the spring the less can it lag behind. This lagging behind is, of course, a turning of the wheel on its axis, relatively to the frame, in the opposite sense to that in which the frame is turned. As we continue to turn the frame with uniform speed, the spring brings the wheel back to its original place against the stop, and further rotation at the same rate makes no change in the relative position of the parts of the machine. But if we now quickly bring the frame to rest, both wheels in virtue of their inertia tend to continue their rotation: one, that one which made the relative movement before, cannot continue its rotation because of its stop; the other can rotate a little, not much, because of its spring—it turns a little, but is soon brought back to its original position against its stop by the spring. You will easily see that just as in the model the inertia of the wheel corresponds to the inertia of the fluid, so here the stretching of the spring corresponds to the stretching of the ampulla. All that we want to make the model complete is to find some way of making the stretching of the spring visible, something which shall correspond to the message sent to the brain. You cannot easily see the stretching of the spring while the frame is turning round, and it was necessary to devise some way of making it visible. We must here leave the analogy of the living organ. The brain turns with the labyrinth, but we are the brain of this machine, and we do not turn with it. After a good deal of consideration, and after thinking of and rejecting a good many plans, I fell upon the one I shall now show you. In the lower end of the axle of each wheel there is fixed a stop-cock, through which gas can pass from one pipe to another. When the wheel is against the stop only a very little gas passes—just enough to prevent the jet going out. When the wheel turns away from its stop, the stop-cock is opened, and the stop-cock is so adjusted that the quantity of gas passing shall be roughly in proportion to the stretching of the spring. By a contrivance indicated in the diagram, the two gas pipes, one from each stop-cock, are brought through the axle of the frame and led each to a gas jet.



Now, when I begin to turn the frame one jet flares up, but as I continue the turning, as nearly uniformly as I can, you see the jets remain at their minimum, which I shall call zero of sensation. Of course I could have made this zero the zero of gas too, but then we should have needed a subsidiary flame to light the gas when the stop-cock opened. I now stop the frame, and you see the other jet flare up for a little. That corresponds to the secondary imaginary rotation which we feel when a real rotation is stopped. I ought to apologize for so often calling this a secondary or imaginary rotation. I hope you all see now that it is as really an *acceleration* in the strict meaning of the word as the original start from rest.

I have taken this question—What is the organ by means of which we perceive acceleration of *rotatory* motion?—first, because it has been most fully worked out. We now come to the question, How do we perceive acceleration of translatory motion? This, as we have seen, is the same as the question, How do we by our senses recognize the direction and estimate the intensity of what is to us at the moment the force of gravity? A very natural suggestion as to the way in which we perceive the intensity of this force is that it is a skin sensation; that it is by the greater or less pressure which we feel on that part of our body which rests on our support. Prof. Mach, to whose experiments I have had often to refer, and to whom we owe, more than to any other investigator, our knowledge of the whole subject of the sensation of motion, has very clearly proved that this is not the case. Everything we know as to this sense leads us to look for its organ in the head. And there is an organ which, to some extent, at all events, seems to be what we are in search of. The *macula acustica* in the utricle is a spot well furnished with nerves, and we have not found out any special function for it. There is a similar *macula* in the saccule, that other membranous bag contained in the vestibule. Mach has suggested that the *macula* of the utricle may be the organ by means of which we perceive acceleration of translatory motion. Let us look at it, and see how far it is fitted to act as a level. Its length is stated by Prof. Schwalbe to be about $\frac{1}{8}$ of an inch, and its breadth a little less. According to the same observer, it covers a part of the floor, the anterior wall, and a part of the external wall of that part of the utricle called the "recess." Its nerves end, as has already been stated, in hair-cells, and these are covered by a gelatinous substance filled with a fine powder of crystals of carbonate of lime. What is interesting to us in this description is that it looks in three directions, and that the whole of it is covered with a powder of considerably greater density than the fluid (the endolymph) with which the utricle is filled.

Let us try to imagine a model of this structure. Let us take a box of glass, so that we may see what goes on in the inside of it. Let us put on a part of the bottom of the box, on the end of it, and on a part of the side of it, a layer of thin jelly mixed with fine sand. Fill up the box with water, and put on a lid. We shall find that we have an apparatus that does to some extent answer the purpose of a level. When we change its inclination, the jelly but for the sand would indeed have very little tendency to change its position, but the sand, being specifically heavier than the water will, and will either move through the jelly if that is thin enough, or pull the jelly with it. In any case, a change of position of the box will involve a change in the relative position of its contents. In the actual case of the *macula acustica* such a change in the relative position of the sand and the hair-cells must give rise to an irritation of the terminations of the nerves, and send a message to the brain. We cannot as yet work out all this in detail, as we can the way in which the canals give us information as to the acceleration of rotational motion, but we know enough to turn our attention to the subject; and we may hope that, by more accurate study of the sensation phenomena, and by comparison of them with the anatomical facts, this important and interesting physiological question may be satisfactorily answered.

ON THE GEOLOGICAL HISTORY OF THE PREHISTORIC FLORA OF SWEDEN.

FIFTY years have passed since the Danish Professor, Japetus Steenstrup, presented to the world his masterly researches on the history of Denmark's peat-bogs. These researches clearly demonstrated that the forests of Denmark had suffered remarkable variations. The oldest forests had consisted chiefly of aspen (*Populus tremula*), next, for a long period, of Scotch fir (*Pinus sylvestris*), then of oak, and, finally, of alder (*Alnus glutinosa*). Remains of the beech—now Denmark's chief species of tree—are, however, entirely absent from the peat-bog. Consequently, it must have been the last to immigrate. It was a natural surmise—and one even advanced by Steenstrup—that the changes referred to were connected with a gradual softening of the climate, a view defended, too, by Prof. Forchhammer.

But Steenstrup's researches were in advance of their time; glacial geology was only in its infancy, and voices were therefore raised both in Denmark and Sweden claiming to interpret these changes in the forest vegetation as one of Nature's great systems of evolution, whereby one variety, so to speak, prepared the soil for the next comer, without any reference to climate. However, this view has now but an historical interest. For, since our knowledge of the geology of the Glacial age has become more and more enlarged, and since remains have been found in Scania of a true Arctic flora embedded in the fresh-water clay deposits of that province, a return to Steenstrup's theory that the changes of climate and forest vegetation were related was but natural. Indeed, the same Arctic flora was shortly discovered underneath the aspen layer in the Danish peat-bogs, so that the aspen flora cannot be regarded as the first after the Glacial age. By degrees, as the ice melted, the denuded soil was invaded by the Arctic flora from the south. First, when the climate became still milder, and a forest vegetation could flourish, the aspen and the birch immigrated, and in turn the pine, oak, and alder. Each one formed in its day the forest of Denmark, and they were naturally accompanied each by its own peculiar undergrowth of shrubs and plants. Enormous ages elapsed between these events. The remains of the peat-bogs show that generations upon generations of grand firs flourished before the oak immigrated and before the pine flora was at last ousted. And in its turn the oak reigned supreme during countless ages, until that, too, was extinguished by the alder and the beech. During the fir period, the men of the Stone Age spread themselves over the land; when the fir was supplanted by the oak, the Bronze Age began. If we bear in mind how exceedingly slow the extension of the oak and the beech is, we can form some idea of the immense time that must have elapsed since the Ice age.

Therefore the flora of Denmark consists entirely of the offspring of immigrated plants. Many of the species which appeared in the country did not remain; to them Denmark was only a station on the road towards higher latitudes. This was, for instance, the case with the greater portion of the Arctic flora, as well as with the fir. From the beginning of the historical period the beech has flourished throughout the whole country.

In Sweden, in districts rich in calcareous matter, tuff strata are deposited from springs which, by their contents of carbonic acid, contain carbonate of lime. When calcareous water is exposed to the air, the carbonic acid gas evaporates, and the carbonate of lime is precipitated in the form of a white deposit, which soon hardens to stone. Such a deposit is therefore particularly natural around the mouth of a spring or in some pool into which the calcareous water is discharged. Consequently the leaves or other remains of plants growing around are covered, soon after falling into the water, with a thin coating of chalk, and although they are by degrees destroyed, this imprint in the chalk remains—often so distinctly that the finest fibres of a leaf may be traced.

A necessity for the formation of calcareous tuff is therefore the presence of calcareous rocks, whence the water may draw the lime. And, indeed, with us all calcareous tuff deposits, as far as we know, are confined to where the chalk formations are richest, as, for instance, Scania, Western and Eastern Gothia, Jemtland, Ångermanland, and Åsele Lappmark.

The greatest in extent and richest in leaf imprints are the old well-known tuff strata at Benestad, in Scania, north of the town of Ystad. They were formerly largely worked for building purposes, as, for instance, for several churches. The tuff is referred to by early writers as rich in leaf imprints, but in their writings a serious error crept in, viz. that even leaves of the beech were imprinted. This is wholly without foundation.

In consequence of the quantity of stone removed, it is now impossible to fix precisely the nature of the stratification, and what we know on this point is due to the particulars supplied by Baron Claes Kurck, who carefully examined the strata. His researches fully confirm the views of the writer, expressed as far back as 1872, viz. that the oldest strata were deposited whilst the aspen was the predominating tree in the districts, and before the fir had immigrated. Kurck has also found here traces of birch, grey sallow (*Salix cinerea*), and possibly the common sallow (*Salix caprea*). Above this stratum we come to the fir, deposited when that predominated. The imprints of the fir, in the shape of needles, branches, bark, and cones, show that this tree grew close by the springs. Most of the tuff dates from the fir period, but during the same age other species of trees gradually immigrated, of which indications are found in the lower parts of the fir deposit. From these the National Museum possess a rich collection, chiefly made by Nordenskiöld in 1873. It contains several rare species of plants, and the imprints of leaves are so remarkably clear that the collection is one of the greatest ornaments of the palæontological section.

Of the trees which flourished contemporaneously with the fir, we learn from Kurck that the birch, mountain ash (*Sorbus aucuporia*), *Salix caprea*, and *Salix aurita*, were the oldest, and the hazel but little younger. To somewhat later strata he refers dog-wood (*Cornus sanguinea*) and berry alder (*Rhamnus frangula*), and he believes that the remains found of Dutch rush (*Equisetum hyemale*), and the gueller rose (*Viburnum opulus*) also date from the same period. If this be the case, it is probable that some leaves in the Nordenskiöld collection of hawthorn (perhaps *Cratægus monogyna*) also belong to this stratum.